A quick report of `The open ocean sensible heat flux and its significance for Arctic boundary layer mixing during early fall' by Ganeshan and Wu.

The paper was well re-written based on my previous comments. However, following issues should be reconsider before acceptance of this manuscript.
1) Analysis using 2013 data, 2) method of cloud fraction estimation, 3) definition of cloud regimes, and 4) interpretation of the relationship between SSHF and SLP. Detail comments and suggestions were shown below.

The data in 2013 cruise:

According to the reply comment, the authors seemed to try to use 2013 data. The observations were mainly made not only along a single longitude (168W) but also at the fixed point during two weeks (72.75N 168.25W). This period should be very suitable to look at the change in BL height. The observing network during the cruise was described in Inoue et al. (2015), and the information of strong winds and surface heat fluxes was shown in Fig.3 in Kawaguchi et al. (2015). Maximum wind speed was more than 13 m/s, and SSHF during the period was more than 100W/m^2, providing a good sampling range for Figs. 3,4,5,11 and 12 in this manuscript. Please try to use the 2013 data in the analysis and results.

Estimation of cloud fraction:

The cloud fraction is a very important parameter in this study to decide the cloud regimes. However, the method of estimating the cloud fraction is not well described.
Lines 125-126: The definition of cloud fraction in this study is `the percentage ratio of cloud layer thickness within the BL'. I think that this definition is not the cloud fraction but cloud layering and its vertical integration. Please cite relevant references to prove this method correct. Although the authors cited Sato et al. (2012) to estimate the cloud layer, Sato et al. (2012) used the 90% relative humidity as a threshold to detect the bottom height of cloud layer (they did not estimate the cloud fraction).
Lines: 127-129: Please show the cases of cloud fraction with 30, 50, and 80 %

instead of the extreme case (i.e. 0%). Ceilometer data used in Sato et al. (2012) would be suitable to calculate the hourly and/or daily effective cloud fractions by averaging 1-min data. Visual observations of cloud cover would be also available in the cruise report at radiosonde section. These data would be used to validate the cloud fraction estimated in this study.

Effects of the surface pressure:

The authors pointed out the relationship between SLP and SSHF/BL height. However, SLP is not a suitable physical parameter to modify the SSHF and BL height. Wind speed and temperature advection induced by SLP and temperature gradients should be essential to understand the linkage of SSHF /BL height.

Dry regime:

Lines 210-212: I can not understand the physical relationship between low SLP and low BL cloud fraction. In case of cyclones, the SLP is low with high cloud fraction. What kind of atmospheric situation is considered in the dry regime ?

Medium cloud regime:

Line 214: How did authors confirm the stratocumulus-topped boundary layer ?

Line 220-224: One of the reasons why the correlation of SSHF and BL height was low is that a cloud layer is not always detected by each profile because cloud cover during CAA is relatively low. Therefore, larger variability of temperature in dry regime (e.g. Fig. 10a) came from inclusion of CAA cases.

Line 243: The procedure to estimate the cloud fraction is doubtful, suggesting that the results heavily depend on cloud fraction method. Therefore, the correlation shown here is not so meaningful.

Lines 244:247: By definition, delta T is a main factor for SSHF shown in (Eq. 1). And I do not agree that boundary layer mixing is strongly controlled by

the surface pressure. There is no physical process between BL mixing and surface pressure. The author should mention about wind speeds instead of SLP.

Lines 268-269: Here, the definition of stratus and stratocumulus regimes should be described. Fig. 12 is a very different style comparing with Fig. 8. The authors should mention how to calculate occurrence frequency of three cloud regimes, in particular a threshold between stratus and stratocumulus, i.e. the value of 75%.

Lines 299-304: The authors should note logically that SSHF is not an only factor determine BL height. Do not conclude the story just using the correlation coefficient shown in Table 2. In addition to this, Fig. 7 is meaningless because cyclones induce both of southerly warm advection and northerly cold advection associated with the passage of a frontal system. The former generates negative SSHF with lower BL height, while the latter generates positive SSHF with developed BL, suggesting that the wide range of SSHF and BL height makes the correlation coefficient high. Again, SSHF depends on temperature difference and wind speeds as shown in (Eq. 1). Even in high SLP years, wind speeds were high because the ship might locate at the edge of the Beaufort high. Therefore, the logic which explains the relationship between SSHF and SLP would be totally incorrect.

Other comments:

Lines 325-326: In general, the cloud-driven turbulence is formed by radiative cooling at the cloud top. I can not find such explanation related to cloud-driven turbulence in sections 4.2 and 4.3.3.

Line 333: In general, the cloud cover is very high in case of cyclones.
Again, what kind of atmospheric state is considered under the low surface pressure under clear sky ?

Line 347-352: I think that the threshold value of cloud fraction between

stratus and stratocumulus is very arbitrariness (75%: Table 2). The discontinuous value to divide the frequency distribution in Fig. 8a must be between 85 % and 95 %. Please recalculate the all parameters and figures including correlation coefficients shown in Tables; otherwise I do not agree with the content in this paragraph.

Line 365: Please explain the physical processes related to sea level pressure.

Line 368: What is a low-pressure condition ? A strong cyclone or frequent passages of weak cyclones ? The author should look at the weather charts in 2002.

Line 380: I can not imagine the cloud-free and low-pressure conditions. Is it a real physical condition to explain the results ?

Lines 386-391: At first, the authors should reconsider the method to estimate the cloud fraction.

Reference list:
Line 39: Nicholls and Leighton (1986) is missing from the list.
Line 397: Grønas and Skeie (1999) is missing from the list.
Kalnay et al. (1996) is not cited in the text.
Line 398: Pagowski and Moore(2001) is missing from the list.

Relevant references:
Inoue et al. (2015), Additional Arctic observations improve weather and sea-ice forecasts for the Northern Sea Route. *Sci. Rep.*, 5, 16868.
Kawaguchi et al. (2014), Fixed-point observations of mixed layer evolution in the seasonally ice-free Chukchi Sea: turbulent mixing due to gale winds and internal gravity waves. *J. Phys. Oceanogr.*, 45, 836-853.